A New Initiative in Transport Sciences

Executive Summary

The US Transport Task Force has conducted a comprehensive, year-and-a-half-long study of transport science in the US fusion sciences program, assessing progress to date, scientific status, and needs for the future. This study has concluded that the significant and impressive progress realized in understanding and controlling turbulence and transport over the past 15 years has applied primarily to ion thermal transport, and that significant new resources are needed if progress is to be made in areas crucial to the success of fusion where progress has lagged. While major questions in electron thermal transport, H mode/pedestal physics, particle transport, and momentum transport must be answered, the areas of electron thermal transport and H mode/pedestal physics are particularly ripe, in terms of scientific maturity, for a major initiative aimed at answering these questions. These areas are ready because a mature conceptual grasp of the nature and facets of these problems now exists, detailed technical pathways for tackling the problems have been formulated, and the necessary diagnostic, theoretical, and computational tools either exist or the knowledge to build and implement them has been acquired. The US Transport Task Force therefore calls for a new transport initiative, noting that such an effort is scientifically compelling in its own right, and also would contribute in fundamental ways to the success a burning plasma program. A preliminary costing analysis puts the puts the cost at about $14M/year over a five-year program. Likely deliverables are outlined in an appendix.

Brief History

After extensive study by the Transport Task Force and discussions in the wider fusion community, we propose a new initiative in transport science to acquire the tools and capabilities needed for attacking key, outstanding transport problems. The hardware, intellectual, and man-power capabilities presently available in experiment, theory, and modeling, have brought remarkable progress, but largely in one area, *ion thermal transport*. They are not configured to solve other transport problems equally critical to the success of fusion, and equally compelling scientifically. A preliminary costing analysis described later indicates that to successfully tackle the important problems in electron thermal transport and H mode/pedestal physics outlined below, the initiative will require an investment of about $14M/year for 5 years. With a budget that is already overextended in its commitment to crucial problems, we believe the initiative must attract an increment of new funding to the fusion program.

The importance of transport has been repeatedly called out in high-level studies by national fusion and science policy panels. Understanding turbulence and transport through comparison of well-diagnosed experiments, theory and simulation is the first objective listed for national fusion research in the Integrated Program Planning Activity Report, with an ambitious 10-year target to develop a predictive model for key aspects of burning plasmas using advances in theory and simulation benchmarked against a comprehensive experimental database of tokamak stability, transport, particle interaction, and edge effects. To achieve success in transport science, it is essential to characterize local fluctuations and transport in toroidal plasmas, to understand basic mechanisms responsible for transport, and ultimately, to control these transport processes. These goals must be pursued in multiple areas, including ion thermal transport, electron thermal transport, particle transport, momentum transport, and the physics of the H mode and pedestal. They also must be pursued by applying tools and approaches that range from simple to complex, in models, simulation, and experiment.

In 1988 DOE implemented an earlier transport initiative, which placed transport as the top priority for the magnetic fusion program and reprogrammed sponsored research activities budgeted at approximately \$25M/year. The greater fraction of this amount (~\$15M/year) supported transport studies already underway. The remainder funded the development and implementation of a new generation of fluctuation and transport diagnostics, and new theoretical and numerical investigations. These diagnostics opened a window on ion scale fluctuations, providing the first evidence that ion thermal transport was anomalous, and revealing fluctuation characteristics that helped forge a consensus as to the type of fluctuation responsible for the anomaly. Significant and impressive advances have followed, including the ability to routinely control anomalous ion thermal transport and reduce it to the level of neoclassical transport. This has allowed the fusion triple product in individual confinement devices to be increased by as much as a factor of 30 since achieving the H (High Confinement) mode. Progress in controlling ion thermal transport has been singled out by the NRC panel on burning plasmas as a key indicator of readiness to proceed to a burning plasma experiment.

Present Status

However, significant problems in transport remain unsolved. Figure 1, roughly quantifies progress in the five broad areas of transport in terms of the three steps that must be achieved for success. The areas are ion thermal transport, electron thermal transport, particle transport, momentum transport, and H mode/Pedestal physics. The steps are characterizing, understanding and controlling turbulence and transport. Advances in ion thermal transport appear as a ridge at the left edge of the figure. The raised feature in the upper right hand corner denotes the ability of virtually all tokamak experiments to routinely achieve H mode. In the remaining areas the degree of progress is significantly lower. There is wide consensus that progress in the areas of electron thermal transport and particle transport seriously lags behind progress in ion thermal transport. The area of momentum transport is even less developed. In the area of H mode/Pedestal Physics

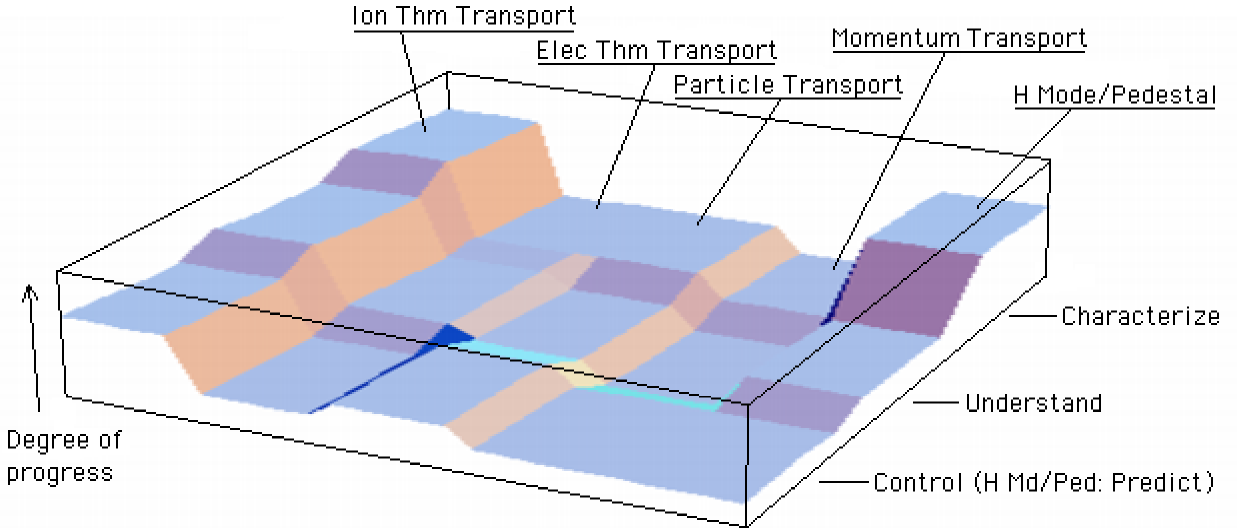

Fig 1: Progress in Transport

there is virtually no predictive capability (which is quantified in the row labeled *Control*). While the physics of turbulence suppression in H mode is thought to be understood, the physics of the L-H transition and the pedestal, which involves the interplay of suppression and several other complicated nonlinear processes, is not understood. The lagging areas pose fundamental scientific questions of the highest order that directly relate to the understanding and control of plasma turbulence, a ubiquitous phenomenon in the universe. Their resolution would directly advance fusion, by allowing the achievement of essential performance objectives including the fusion triple product, operation in burning plasma regimes with strongly coupled electron and ion channels, divertor exhaust control, density profile control, fueling, ash removal, and control of turbulence in steady state operation.

Progress in transport has been uneven, firstly because the window on ion scale fluctuations opened by the original transport initiative brought rapid progress in that area. Secondly, nature has rendered ion scale fluctuations susceptible to flow shear, a quasi-universal, fluctuation-independent suppression mechanism. The unevenness of progress offers strong evidence that existing diagnostics, theory and modeling capabilities are not adequate for solving the remaining problems.

How other fields have responded to deficiencies in understanding is instructive. Consider the solar neutrino problem, which arose when the Homestake detector measured a solar neutrino flux that was 30% of the value predicted by the standard solar model. This modest discrepancy induced a major response and commitment of new resources, spurring investments in detectors, theory, and analysis. The problem inspired refinements to the solar model and ground breaking theory, leading to the discovery of neutrino oscillations and the realization that neutrinos have mass. Importantly, major new detectors were built to join the search for the missing neutrinos. If the absence of half the predicted neutrino flux is worthy of such a response, the absence of electron transport reduction when ion transport is virtually shut off is also worthy of a significant response. So too are the absence of an electron fluctuation signal in the presence of a large anomalous electron heat flux, and other conundrums lurking in the lagging areas of Fig. 1.

Time to Act

Given new developments and recent advances, the time to mount the next transport initiative is ripe. The existing suite of ion scale fluctuation diagnostics, while still crucial for detailed studies to validate the ideas underlying progress in ion thermal transport, is ill posed to attack the other problems in Fig. 1, for which diminishing returns are already in evidence. Answers to these problems are needed if fusion is to move forward. The diagnostics required for tackling these problems, once beyond our technical grasp, are now underpinned by well-developed ideas and techniques for their design, realization, and implementation. Computational physics has crossed a threshold where it is thought that for the first time codes can now incorporate the relevant physics for quantitatively describing transport in a fusion device. Moreover, it is believed that a rudimentary understanding of many of the basic processes underlying fusion transport is in hand. The application, exploitation, and validation of these advances require commensurate advances in diagnostic capability. At present this capability is seriously underdeveloped compared to computation. For example, in the area of electron thermal transport, a flourishing of theory and computation has put forward many new ideas, some controversial, which cannot be tested in experiment because the necessary diagnostics are lacking.

An initiative in transport studies, while compelling in its own right and crucial under any conceivable path forward in fusion, complements other initiatives that have been discussed within the US fusion program. In a US program with ITER participation, the transport initiative represents an ideal base program activity. It could form the basis for or become a key contributor to the US role in ITER. It supports research on key issues for successful burning plasma operation, and will impact ITER performance and the ITER experimental program. It also addresses longer-term fusion issues of concern for potential next step devices, and would thus impact the conception and design of such a device. In any push toward a burning plasma experiment, it is critical that the OFES program maintain a proper balance between basic research, development, and studies relevant to burning plasmas. The transport initiative advances basic science in a way that is readily apparent and also capable of utilizing the OFES portfolio of innovative concept devices. The transport initiative would complement any initiative in computing. Even now, gains in computation are outstripping the ability of experiments to verify and validate the results of numerical modeling studies. This situation will be aggravated if research priorities single out computation without balanced efforts to improve diagnostic capability. In this regard it should be noted that the theoretical discoveries that explained the neutrino shortfall (which are arguably simpler than the mechanisms operating in plasma turbulence) were not deemed a solution until validated experimentally.

There is broad consensus that the focus of the transport initiative should be electron thermal transport and the physics of the H mode and pedestal, although, should a compelling idea for addressing another transport area emerge, it too should be considered. These areas are important in current experiments, but become even more so in future experiments. Key diagnostics required for progress in these areas have been identified and are ready for development. Present understanding in these areas has reached a threshold in maturity that has allowed the development of detailed roadmaps for progress. Groups and individuals are taking the initial steps of these roadmaps, albeit with limited resources. The technical addenda that follow lay out the specifics for each of these areas, including reasons for importance, outstanding questions, new diagnostic capabilities needed, and needed developments in analysis, theory, and modeling. There are also addenda on the status of diagnostic development required for progress in these areas, the deliverables anticipated from a transport initiative focused in these areas, and the activities in TTF and the fusion community that have led to the present document.

Costing Analysis

A rough estimate of the cost of a transport initiative was obtained using a simple algorithm that indexes all transport initiative activity on existing machines to diagnostics. That means that additional experimental runtime and overall manpower, including that of theoretical and computational efforts required to maintain a balanced scientific effort in a program that is embarked on a phase of major discovery, are calculated as multipliers to the number of diagnostics. The multiplier admits one senior and one junior staff researcher in a national experimental facility setting, one senior and one junior staff researcher in a university setting, 4 postdoctoral, and 4 graduate student researchers. These individuals represent appropriate combinations of experimental, theoretical, and computational manpower. Diagnostics themselves are estimated to cost an average of \$2M total (phased over 3 years), with operating costs of \$200,000/year once the diagnostic is operational. The diagnostic cost represents an average over diagnostics ranging over diagnostics requiring major development to more routine fluctuation and profile diagnostics. It also includes development of associated analysis techniques where desirable. 14 diagnostics are envisioned, applied to appropriate components of the OFES portfolio of devices, in a number and manner dictated by scientific objectives. The costing analysis also provides for one new small-scale experiment dedicated to basic scientific studies of turbulence, at a cost of about $6M over the 5-year period. The costing analysis is approximate and designed only to give a rough estimate.

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Signed on behalf of the TTF Executive Committee: Boris Breizman, Bill Dorland, Ken Gentle, Chuck Greenfield, Rich Groebner, John Kinsey, David Mikkleson, Rick Moyer, Rafi Nazikian, William Nevins (Vice Chairman), David Newman, and Ed Synakowski (Chairman, Steering Committee)

Addenda Including Technical Detail

Addendum 1. Activities conducted in preparation for transport initiative proposal

Reports on Status of Transport Areas

By invitation of the TTF Executive Committee, these reports were prepared by experts and presented in recent TTF meeting plenary session as 45 minute preview talks, followed by 45 minutes of discussion. The viewgraphs used in these presentations are accessible by going to the indicated URLs and downloading PDF files.

"Issues in Our Understanding of Electron Thermal Transport", P.H. Diamond, Annapolis, April 2002, http://www.psfc.mit.edu/ttf/2002/invited_talks/agenda.htm

"Electron Thermal Transport: an Experimental Perspective", Francois Ryter, Madison, April 2003, http://www.psfc.mit.edu/ttf/2003/previews/agenda.html

"L-H and Pedestal Physics Issues", Amanda Hubbard, Annapolis, April 2002, http://www.psfc.mit.edu/ttf/2002/invited_talks/agenda.htm

"Effect of Plasma Flows on Turbulent Transport and MHD Stability", K.H. Burrell, Annapolis, April 2002, http://www.psfc.mit.edu/ttf/2002/invited_talks/agenda.htm

"Particle Transport", Mickey Wade, Madison, April 2003, http://www.psfc.mit.edu/ttf/2003/previews/agenda.html

"Magnetic Fluctuations and Transport", Xavier Garbet, Madison, April 2003, http://www.psfc.mit.edu/ttf/2003/previews/agenda.html

"A Burning Plasma Experiment and its Relation to Transport Science", Jack Connor, Annapolis, April 2002, http://www.psfc.mit.edu/ttf/2002/invited_talks/agenda.htm

"The Experiment/Theory Dialogue in the Age of Simulations", W.H. Nevins, Annapolis, April 2002, http://www.psfc.mit.edu/ttf/2002/invited_talks/agenda.htm

"Towards Obtaining a Predictive Capability for Transport", G. Tynan, Madison, April 2003, http://www.psfc.mit.edu/ttf/2003/previews/agenda.html

Transport Initiative Town Meeting, November 14, 2002, Orlando (satellite meeting at APS DPP 2002)
Outlines of transport initiative proposal were presented followed by 90 minutes of discussion.

Transport Initiative Panel Discussion, April 4, 2002, Annapolis
Representatives of major experiments were invited to discuss their future plans for transport studies and how studies might be better coordinated between devices.

Transport Initiative Open Forums, April, 2002, Annapolis; November, 2002, Orlando
Discussions of technical readiness and diagnostic status were held among interested members of transport community as part of open discussion time in working groups.

Presentations and Discussions at Fusion Labs and Facilities (conducted by P.W. Terry)
General Atomics, January 2003
Lawrence Livermore National Laboratory, January 2003
University of Wisconsin-Madison, February 2003
Plasma Science and Fusion Center, M.I.T., February 2003
Princeton Plasma Physics Laboratory, February 2003
OFES Budget Meeting, March 2003
University of Texas at Austin, May 2003

Addendum 2. Electron thermal transport

A. Importance of electron thermal transport problem
   1. In plasmas with $T_e \geq T_i$ (e.g., burning plasmas) $\Rightarrow$ confinement time can be limited by electrons
   2. Becomes dominant heat loss channel when ion transport and fluctuations are reduced; may indirectly drive particle and impurity transport
   3. Exacerbated by
      a. Use of ion transport barriers making χ_i small
      b. High β, favoring magnetic fluctuations
      c. Steady state operation with electron heating
   4. Must have transport barriers for both χ_e and χ_i

B. Status: Like χ_i problem ca. 1985, but *without fluctuation measurements* ⇒ lots of eligible models, hints at control strategies, no consensus on characterizing, understanding, controlling

C. Outstanding problems

- Not known which candidate fluctuation type causes χ_e anomaly
- χ_i reduced in ITBs (internal transport barrier); with pellet density peaking; χ_e not reduced
- Cause of χ_e anomaly in ITBs with χ_i reduction not known
- Electron ITBs exist with electron heating, but χ_i not reduced
- Mechanism for χ_e reduction in electron ITBs not understood
- No definitive measurement of fluctuations at high wavenumber $\rho_i < k^{-1} < \rho_e$
- Not known if magnetic turbulence does or does not play role
- Limited diagnostic window on magnetic turbulence
- No χ_e model has been ruled out, including global scale magnetic turbulence, trapped electron modes, microtearing fluctuations, ETG
- Theory/simulation has postulated many ideas yet to be tested in experiment, including fluctuations on ρ_e, c/ω_p scales, streamers, zonal flows, cascades from ρ_e to c/ω_p scales
- No consensus in simulations about existence and magnitude of crucial structures (e.g., streamers, zonal flows) for ρ_e scale fluctuations
- No theoretical consensus on nonlinear dynamics of ETG, e.g., role of zonal flows, streamers, secondary and tertiary instability, linear versus nonlinear instability
- Certain possible mechanisms (intermittent global scale magnetic turbulence) have not been developed to predictive level
- Possible role of nonlocal effects near marginality not established

D. Needed measurements

1. Measure high wavenumber spectrum
   - Almost no existing diagnostics - requires diagnostic development
   - Any measurement with $k\rho_i > 1$ interesting
   - Ideally measure $\rho_i < k^{-1} < \rho_e$, including c/ω_p scales
   - Measure spectrum anisotropy in different ranges
     $k_r < k_\theta$ for $k\rho_e \sim 1$? → streamers
     $k_\theta < k_r$ for $k\rho_e < 1$? → zonal flows
   - Measure k_r spectrum of $k_\phi = k_\theta = 0$ – does it extend to c/ω_p, ρ_e ?
   - Measure radial correlation – is Δr of flux different from Δr of fluctuations? → avalanche vs. streamer
   - Power laws? – cascade vs. local source
2. Measure short wavelength bispectrum, bicoherence
   - c/ω_p excited by inverse cascade from ρ_e ?
   - $k_\theta < k_r$ driven by $k_\theta \sim k_r$ of smaller scales? – zonal flow drive via inverse cascade

        - $k_r < k_\theta$, if present, driven by 3-wave coupling? (otherwise $\Rightarrow$ linear instability)
        - Inverse cascade stops at c/ω_p or continues to $k < (c/\omega_p)^{-1}$?
        - Any high k from larger scales via forward cascade?
    3. Measure nonlinear growth rate

$$\gamma_{nl} = [2E(k)]^{-1} \left\{ \frac{dE(k)}{dt} - \left. \frac{dE(k)}{dt} \right|_{3\text{-}wave} \right\}$$

$E(k)$: fluctuation energy

$\left. \frac{dE(k)}{dt} \right|_{3\text{-}wave}$: summed bispectrum of nonlinearities

        - Check secondary instability vs. primary instability for streamers
        - Role of stable modes (like GAM) on spectrum
        - Direct measurement of marginality (in linear growth or nonlinear growth?)
    4. Magnetic fluctuation scan – single device ($q < 1$)
        - Scan from magnetic to electrostatic fluctuation dominated discharge
        - Examine χ_e vs. D
        - Examine χ_e vs. χ_i
        - T_e profile
        - Wavenumber spectrum – does power law go away?
        - Bispectrum in δb
        - Runaway confinement
        - Study region of marginal island overlap
    5. Magnetic fluctuation scan, multiple machine (β scan, scan of δb importance)
        Comparative measurements in different machines (e.g., low q plasma - low aspect ratio plasma - conventional plasma)
        - δb spectrum (FIR polarimetry)
        - χ_e vs. D
        - T_e profile

E. Needed theory and modeling
    1. Simple models for analytic treatment of structure formation, evolution (streamers, zonal flows, etc.)
        - Understand mechanisms for formation
        - Understand conditions for formation
        - Obtain scalings with relevant parameters for comparison to exp.
    2. Develop code diagnostics that replicate experimental measurements
        - Correlations, fluxes
    3. Develop theory for saturation of large-scale magnetic turbulence (“bubbling”)
    4. Electromagnetic and real geometry effects on ITG/ETG/TEM modes, especially near edge
    5. Replicate experimental values of appropriate Reynolds number - e.g., reconnection of stream lines due to unrealistically large viscosity?
    6. Integration
        - Detailed studies of nonlinear effects of nonadiabatic electrons, ions

    - Coupled ion, electron mode evolution in ITB
  7. Systematic resolution studies and tests
  8. Serendipitous research - entirely new theoretical model needed?

F. Comparisons of theory and experiment
  - Comparisons almost nonexistent!
    - Theory has developed largely independently of experiment
  - Detailed comparisons are crucial for solving problem
    - e.g., success in attacking ion problem
  1. Are there revealing comparisons possible with existing diagnostics?
  2. Are there revealing comparisons possible with modest diagnostic extensions?
  3. Are there advantages to be gained with special conditions?
    - e.g., Machine with low $B \Rightarrow$ smaller $\rho \Rightarrow$ access electron scales at lower k

G. Necessary approach
  1. Experiment, theory and modeling must range from simple to complex. Difficulty, uncertainty, limited access, and restrictions attendant to data collection and modeling in complex environments needs to be balanced by the tractability, reliability, enhanced diagnostic access, and ability to control and scale experimentation and modeling in simple environments

Addendum 3. The physics of H mode and pedestal

A. Importance of H mode, pedestal physics problem
  1. H mode crucial for tokamaks, burning plasma experiments
    a. Confinement improvement
    b. Optimal fusion power
    c. For burning plasmas, Q strong function of pedestal temperature
    d. Sets edge boundary for stiff core temperature gradient
    e. Used in combination with ITB (quiescent double barrier)
    f. Can enhance other parameters

B. Status: routine production ⇒ extensive empirical data base
  1. Good evidence that sheared E_r leads to reduction of turbulence transport
  2. Empirical threshold power scalings
    a. Role of density, toroidal field, ∇B drift direction
  3. Characterized as bifurcation with hysteresis
  4. Empirical pedestal height scalings
    a. Role of plasma current, shaping, density, etc.)
  5. Emerging sense that SOL physics must be included to understand pedestal
  6. Some manipulation of ELMs
  7. Encouraging success in ELM modeling via MHD
    b. Pedestal pressure gradient seems limited by balloon/peeling modes (but process determining width is unknown)

C. Outstanding problems
  - Embarrassment of riches: multiple parameters, triggers, conditions ⇒ many models (33 local, model threshold criteria; 22 model power threshold scalings)
  - Fundamental concepts, equations (e.g., small gyroradius, neoclassical theory, gyrokinetic theory) not valid for steep gradient region of pedestal

- No consensus on correct model, many processes active (e.g., orbit losses, strong electric fields and shear flow, neutral fueling, nondiffusive transport, MHD limits, divertor geometry, open field lines outside separatrix)
- No consensus on model input landscape, i.e.,
  – Which parameters, physics inputs robust, which are not?
  – Different mechanisms under different conditions, or single mechanism?
  – Which parameters must be included to get general workings right? to get accurate predictions?
- No validated predictive capability of when transition will occur
- No validated predictive capability of how barrier will evolve
- Threshold dependence on ∇B drift direction not understood
- Not known what determines pedestal width
- Not known what determines pedestal height
- Correct magnitude, scalings for pedestal width and height unknown
- Not know what governs particle and momentum transport in barrier
- Not known what sets pressure gradient limits in all ELM regimes, despite significant progress in some regimes with peeling/ballooning model

D. Needed work

1. Expand and improve experimental database as groundwork for developing predictive capability
   a. Improve quality of edge profiles, particularly for pressure gradient
   b. Study width and gradient separately for T_e, T_i, and n_e profiles
   c. Develop a measurement of edge current density
   d. Measure mode numbers of ELM precursors
   e. Improve characterization of spatial and temporal behavior of turbulence in pedestal
   f. Measure particle flux through pedestal region
   g. Scale width and gradient separately
   h. Measure particle source over full poloidal cross section of plasma
   i. Assess nature and role of edge magnetic topology
2. Assess robustness of parameters
   a. New analysis techniques
   b. Extend range of variation of parameters
3. Apply more stringent tests to transition models
   a. To cull models, use specific experimental conditions to determine dominant instabilities, dominant suppression mechanisms in pedestal region
   b. Extend most promising models to get specific, testable predictions
      – Driving flux threshold
      – Transition location
      – Local threshold parameters
   c. Test predictions against high-resolution data
      – Over range of conditions
      – On multiple machines
      – Against larger parameter set from expanded number of diagnostics
4. Develop more complete, self-consistent models of pedestal evolution

    a. Tracing heuristic role of single scale length inadequate
    b. Must include energy flux, particle flux, temperature profile, density profile
    c. Include realistic treatment of edge plasma, e.g., divertor geometry, neutral fueling via appropriate codes
    d. Validate existing fluid turbulence models for pedestal with experimental measurements, comparing kinetic profiles and measured turbulence characteristics
    e. Develop valid kinetic transport theory for pedestal which incorporates turbulent and neoclassical physics
        – Include turbulent electron heat transport
        – Include neoclassical (and turbulent) ion heat transport
        – Include turbulent and neoclassical particle transport
    f. Determine key aspects of models to be checked against experiment and perform the needed measurements

5. Develop integrated models for pedestal structure
    a. Include realistic heat flux from core
    b. Include realistic 2D particle source from SOL and PFCs
    c. Include good treatment of electron heat transport, ion heat transport and particle transport calculations
    d. Integrate MHD stability into pedestal models to determine cyclic heat behavior of ELMs, and consequent heat pulses to divertor
    e. Develop mechanisms for testing models against experimental data from several machines

Addendum 4. Emerging Diagnostic Technologies for Fluctuations and Transport

1. Far Infrared, forward scattering for measuring intermediate *k*
    Purpose: Electron scale density fluctuation measurement
    Sensitivity: Presently being calculated, likely sub percent
    Resolution: 10-20 cm^{-1}, and very low *k* (< 1 cm^{-1})
    Localization: 5-10 cm for 10-20 cm^{-1} wavenumbers
    Enabling Hardware: High frequency (288 GHz), high power sources
    Enabling Techniques: With beam dump, high contrast between wavenumber ranges
    Limitations:
    Status: Under development for implementation on DIII-D

2. Far Infrared, backscattering for high *k* measurement
    Purpose: Electron scale density fluctuation measurement
    Sensitivity: presently being calculated, likely better than sub percent
    Resolution: 40 cm^{-1}, radial wavenumber
    Localization: chord averaged to point of resonance
    Enabling Hardware: 100 GHz, high power sources
    Enabling Techniques: Measurement of backscattered signal

Limitations: ECH port would allow better localization, poloidal wavenumber measurement
Status: Under development for implementation on DIII-D, NSTX

3. Phase Contrast Imaging for high *k* measurement
   Purpose: Electron scale density fluctuation measurement
   Sensitivity: $\int \tilde{n}\, dz \sim 10$ m^{-2} (C-Mod)
   Resolution: 2 cm^{-1} $< k <$ 100 cm^{-1}
   Enabling Hardware: Fast digitizers
   Enabling Techniques: Larger scattering angle
   Feature: Image footpoint of ITB
   Limitations: Sensitive only to k_z=0
   Status: Under development on DIII-D, Alcator C-Mod

4. Microwave Reflectometry for high *k* measurement
   Purpose: Electron scale density fluctuation measurement
   Sensitivity: Much better than $\tilde{n}/n = 10^{-3}$
   Resolution: $k_r \sim 30$ cm^{-1} (NSTX)
   Localization: 4 cm (radial)
   Enabling Hardware: high power, 1 mm probe source; focal plane imaging array
   Enabling Techniques: equatorial plane scattering geometry
   Limitations: Fluctuations at single radius measured
   Status: Not funded

5. Far Infrared Polarimetry
   Purpose: Core magnetic fluctuation measurement
   Sensitivity: 1 Gauss
   Resolution: $k \sim 1$ cm^{-1} (×10 improvements possible)
   Localization: chord separation – 8 cm at present (MST)
   Enabling Hardware: sensitive detectors of rotation of wave polarization
   Enabling Techniques: Faraday rotation
   Features: Also measures equilibrium current profile
   Limitations: Line of sight (chord average) measurement (overcome with structure function), large scale only
   Status: Funded only for operation on MST

6. Beam Emission Spectroscopy velocity fluctuation measurement
   Purpose: Simultaneous core measurement of ion scale density and potential fluctuations
   Sensitivity: ~ 1% of thermal velocity (DIII-D, present capability)
   Resolution: 1 cm per channel (32 channels)
   Localization: 6 cm (radial) × 7 cm (poloidal)
   Enabling Hardware: Diagnostic neutral beam
   Enabling Techniques: time delay estimation analysis
   Features: 2D imaging, flow diagnostic
   Limitations: Limited to ion scale fluctuations, outer part of core

Status: Under development on DIII-D

7. Heavy Ion Beam Probe
   Purpose: Simultaneous core measurement of ion scale density and potential fluctuations
   Sensitivity: 0.1 percent (ñ/n); 3 Volts rms (ϕ) (TEXT)
   Resolution: wavenumbers up to 3 cm^{-1} (TEXT)
   Enabling Hardware: Diagnostic ion beam, accelerator
   Localization: 1 cm (TEXT)
   Features: Magnetic fluctuation measurement possible
   Limitations: Less effective for high density
   Status: Installed on MST, not funded for use as fluctuation diagnostic

8. Gas Puff Imaging diagnostic
   Purpose: 2D Imaging of edge fluctuations
   Sensitivity: Sees in region 5 eV to 50 eV
   Resolution: 2 mm (C-Mod), 1 cm (NSTX)
   Localization: Outer midplane, 6 cm × 6 cm (C-Mod), 32 cm × 16 cm (NSTX)
   Enabling Hardware: Ultra high speed cameras
   Enabling Techniques: View light from localized gas puffs
   Features: 2D imaging, up to 1 MHz framing rate
   Limitations: 28 frames/shot at present
   Status: Implemented on NSTX and C-Mod

9. Edge Current Profile diagnostic
   Purpose: Measurement of current profile in pedestal region
   Sensitivity: 1% (magnetic pitch angle), 5-10% (current density)
   Resolution: 5mm radial, 10-100ms
   Localization: Outer 20 cm of DIII-D
   Enabling Hardware: Li DNB, Photoelastic Modulator, high-resolution tuneable etalons, high sensitivity photodetectors
   Enabling Techniques: High speed multichannel digital lock-in techniques
   Features: Can also provide pedestal density profile evolution
   Limitations: Less effective for higher density
   Status: Under development on DIII-D

10. Other Diagnostics
    Other diagnostics have been proposed and/or implemented but are not under development or being used at the present time in the US. Among these are electron cyclotron emission for temperature fluctuations, and mode conversion scattering for small-scale magnetic fluctuations. The latter was implemented on Tor Supra. While interpretation of the results was controversial, this diagnostic may be worth further consideration. Probe diagnostics are more standard, and have not been included in the above list, but are nonetheless crucial in fluctuation diagnosis in the edge.

Addendum 5. Likely Deliverables

In a five year period there is a significant probability that the following could be accomplished:

- In the area of electron thermal transport
    - Implement electron scale fluctuation diagnostics on major tokamaks
    - Determine whether electron scale fluctuations are responsible for the anomaly of χ_e, with and without internal transport barriers
    - Determine the fraction of electron thermal transport attributable to magnetic and electrostatic fluctuations in high β, high performance plasma
    - Determine the type of fluctuation or phenomena responsible for anomaly of χ_e
    - Make significant progress on control of electron thermal transport
- In the area of H mode and Pedestal Physics
    - Create and refine integrated H mode/Pedestal models with multiple physical effects (e.g., MHD instabilities, dynamically realistic fluxes, boundary physics, atomic physics, etc.)
    - Perform detailed experimental tests and comparisons
    - Make significant progress toward predictive capability of the height, width, and scalings of the pedestal, and of H mode transition threshold scalings